1974

A Critique of the Report of the Shreveport Experiment

Julius G. Getman
Indiana University School of Law

A CRITIQUE OF THE REPORT OF THE SHREVEPORT EXPERIMENT

*JULIUS G. GETMAN**

IN the January 1973 issue of this journal, Robert Hallauer reported on the American Bar Foundation's study of the "Shreveport Experiment" in prepaid legal insurance.[1] The Shreveport plan has been widely discussed and similar plans are being considered in other locations. Under the plan, legal services for approximately 600 members of Local #229, Laborers' International Union, were paid for up to generous limits which varied according to the service provided. The members paid a premium for coverage and were free to select their own lawyers.

The researchers conducted interviews before the plan began, and after one year of operation, with as many of the covered employees as they could. They also interviewed selected members of the Shreveport Bar before and after commencement of the plan. Their conclusions, while tentative, are uniformly favorable to the plan. They find that it improves selection and increases use of lawyers while giving the individual enrolled in the plan a sense of entitlement to legal services and a greater sense of their value.

Unfortunately, the study has a number of serious deficiencies. They involve recurrent problems in legal field research and hence deserve attention quite apart from the intrinsic importance—which is great—of the issue of prepaid legal insurance. The criticism which follows is meant to be not an attack on the researchers but an example of the problems involved in designing and conducting a study of this important issue. Methodological criticism is relatively rare among legal scholars, but it is necessary if we are to develop a sophisticated understanding of the uses and limits of field research.

THE PROBLEM OF GENERALIZING FROM THE SHREVEPORT EXPERIENCE

The employees who participated in the Shreveport plan were "unskilled, low income construction workers, living just above the federally defined poverty line." Almost all were black, most were over 50 years of age, they had similar educational backgrounds, and they lived in the same area.[2]

* Professor of Law, Indiana University (Bloomington).

[1] Robert Paul Hallauer, The Shreveport Experiment in Prepaid Legal Services, 2 J. Leg. Studies 223 (1973) [hereinafter cited as Hallauer].

[2] Hallauer at 224-25.

Hallauer acknowledged that "the findings cannot be applied to the American population at large" but minimized the importance of this limitation on the ground that

> . . . the sample of legal clients and potential clients studied here represents to some extent that segment of the American population which by virtue of its socioeconomic status, education and occupation is distant from lawyers and generally excluded from full access to legal services.[3]

But are the respondents *really* representative of those generally excluded from access to legal services? What of white unemployed, factory workers, working women, northern slum dwellers, and nonunion workers? What light does the study cast on their potential responses to prepaid legal insurance? Even some groups which are not excluded altogether from the study—such as the young, the single, and high-school graduates—are so minimally represented that no significant generalizations about them can be derived from the study.[4] There is no evidence to suggest that legal needs or responses to such a program would be the same across all of these diverse groups of people who customarily do not seek or utilize legal counsel.

In particular, the fact that the respondents are members of a union separates them from most of the poor and the powerless in our society. Since the experiment was conducted and the plan partly administered through the union, its findings simply cannot be expanded to include the unorganized, who do not have adequate access to lawyers. Nor are the respondents even typical union members. One would not expect the same results to follow automatically if the plan were applied to unions in which the members were white, more highly paid, and more accustomed to using lawyers.[5]

As Mr. Hallauer's article suggests, he and his colleagues initially did not recognize the importance of the fact that their respondents were not typical of the potential clients of a prepaid insurance plan.[5a] They attempted to de-

[3] *Id.* at 225.

[4] Only 10 per cent had no eligible dependents, only seven per cent were under 30, and only eight per cent had 12 or more years of school. F. Raymond Marks, Robert P. Hallauer & Richard R. Clifton, The Shreveport Plan: An Experiment in the Delivery of Legal Services 21 (Am. Bar Foundation, 1974) [hereinafter cited as Marks, Hallauer & Clifton]. But *see* Hallauer at 225, n.5, stating that only two per cent were under 30 and only five per cent had 12 or more years of schooling.

[5] Similarly, it would be unsafe to assume that the response of the Shreveport bar to the plan would be typical of that of lawyers in larger cities or in county seats. The Shreveport Bar is largely homogeneous in terms of race, education, background, and style of practice. They are white, educated in Louisiana, and practice either by themselves or in small firms. There is little specialization.

[5a] See also Raymond Marks and Robert P. Hallauer, An Analysis of the Shreveport Pre-Paid Legal Service Plan After One Year of Operation, Report to the Ford Foundation 14-15 (1972).

sign a study of sufficient breadth to permit general conclusions to be drawn about the desirability of prepaid insurance plans. The later report on the study published by the American Bar Foundation shows greater sophistication in this regard. It points out that "the peculiar demographic and geographic characteristics of the covered group and locale . . . make it almost impossible to generalize from the data. . . ."[5b] If this had been recognized earlier, then, as we shall see, a more focussed and therefore more useful study might have been designed.[5c]

The Failure to Use Control Groups

As Hans Zeisel recently stated in this journal,

> The basic tool (and also the paradigm for retrospective analysis) is the controlled experiment. Its essence is simple: The objects or people on which the experiments are to be performed are divided randomly . . . into two groups; the experimental treatment is then applied to the one group and withheld from the other, the control group. If an effect is found in the experimental group that is absent from the control group it can be attributed to the experimental treatment.[6]

Professor Zeisel pointed out that opportunities for controlled experimentation in the law are rare and alternative techniques frequently have to be devised. But the Shreveport plan offered an excellent opportunity for controlled experimentation. No legal or practical barrier would have prevented dividing the members of the local into two groups, one covered by the plan and one not. Perhaps there would have been objection to such a procedure within the union, but in that case it might have been possible to find another local whose members were comparable to Local #229 in race, income, age, and use of lawyers, and could therefore have served as the control group.

The researchers did not use a control group. Neither the Hallauer article nor the subsequent volume on the plan published by the American Bar Foundation[7] discusses this decision, but apparently it was assumed that a control group was rendered unnecessary by the use of panel design.[8] This assumption constituted a major error.

[5b] Marks, Hallauer, & Clifton at 7.

[5c] After Mr. Hallauer's article appeared, I was invited to discuss it and the study which it reported. At that time I also received a copy of the report by Hallauer and Marks describing their methodology and results to the Ford Foundation (see note 5a *supra*). After my article was submitted, the Bar Foundation Report (*supra* note 4) was published. I was given the opportunity to examine the page proofs of that report prior to the publication of this article. I have tried to eliminate criticisms which are corrected by the final report.

[6] Hans Zeisel, Reflections on Experimental Technique in the Law, 2 J. Leg. Studies 107 (1973).

[7] See Marks, Hallauer & Clifton.

[8] The respondents were interviewed before and after commencement of the plan. In

Hallauer reported an increase from 31 to 49 in the number of members taking problems to lawyers during the plan year (10.3 per cent to 16.3 per cent), and stated, "the only obvious change that would account for the increased rate is use of the plan itself."[9] During the preceding five years, however, there had been a growing tendency to utilize lawyers. While the increase during the plan year is described as "greater than one would expect random change to be," in fact a similar increase had occurred two years *before* the plan went into operation.[10] Moreover, during the plan year special factors were operating that make generalizing about the plan's impact particularly hazardous. Respondents who were aware that the plan was soon to be implemented held back on going to a lawyer to await the plan's operation.[11] And the publicity given the plan, including the interviewing, may have sensitized respondents to the use of lawyers, thereby creating a short-term change not due to the specific features of the plan.[12]

A similar error was made in automatically assuming that attitude changes between the first and second interview were attributable to the plan. Respondents were asked four questions before and after the plan to test general attitudes about lawyers. In each case a very slight favorable shift was noted.[13] Hallauer attributed these minor changes in attitude to the plan on the ground that "so far as we know that is the only new circumstance which would have a bearing on legal attitudes."[14] Respondents were also asked how much a half-hour consultation with a lawyer would cost and how much it should cost. Before the plan, 13 per cent of 289 respondents said it would cost nothing and 18 per cent said it should cost nothing. After one year the percentages were reduced to 7 per cent and 12 per cent respectively.[15] Hallauer stated:

---

their report to the Ford Foundation the researchers commented that this technique made "the base line of a control group superfluous." Raymond Marks & Robert P. Hallauer, *supra* note 5a, at 7.

[9] Hallauer at 226. No significance levels are given in any of the tables.

[10] *Id.* at n.6. The number of problems taken to lawyers increased from 30 to 52 during that year.

[11] There was an unexpected six-months delay in the start of the plan. Only 11 problems were taken to the lawyers during that period. Marks, Hallauer & Clifton at 63. Respondents who do not use lawyers might accumulate certain types of nonrecurring problems, such as drawing wills, formal adoption, etc., which would create a large initial need for lawyers. On the other hand, new patterns of use based on such factors as greater familiarity with lawyers, greater awareness of their availability, and greater sense of being able to afford legal help might not yet have developed.

[12] Only one respondent who went to a lawyer indicated he would not have done so but for the plan. Hallauer at 239.

[13] The percentage agreeing that lawyers usually cannot be trusted went from 61 per cent to 57 per cent, agreement that it is hard to find a lawyer when you need one went from 51 per cent to 49 per cent, agreement that most lawyers would not want a black laborer for a client went from 53 per cent to 48 per cent, and agreement that it is hard to understand lawyers went from 77 per cent to 76 per cent. Hallauer at 238.

[14] *Id.* at 239.

[15] *Id.* at 238.

"Admittedly, the numbers involved are small. But this change seems to reflect a falling away of 'charity' or 'paternalism' expectations,"[16] which he attributed to the plan.

Not only are the numbers too small to be significant, but there is no reason to believe that they reflect the impact of the plan.[17] Indeed, there is nothing to indicate whether the respondents who changed attitudes during the plan year had previously used lawyers, had used them for the first time under the plan, or had never used them—even during the plan year. Even attitude changes by those who used legal services paid for by the plan might be due to the legal services themselves, which might have been obtained anyway, rather than to the method of payment provided by the plan. Any changes in attitude by those who did *not* use a lawyer during the year—if related in any way to the plan—would have to be the result of general impressions or of reports by others, and might change after actual experience with lawyers.

In sum, some form of control was necessary to permit statistically sound inferences about the impact of the plan to be drawn from the data.[18]

Problems Created by the Use of Panel Design

Panel design should only be used to obtain information that cannot otherwise be obtained, because the method has serious drawbacks. Its use in the present case sharply reduced the number of respondents about whom useful information was obtained. Although membership in Local #229 at any

[16] *Id.* at 237.

[17] Panel design is generally preferable to a single interview in measuring changes in attitudes and perceptions after an experimental event because respondents often cannot recall their previous state of mind. However, assuming that the changes of attitudes are due to the experimental event is erroneous. For a general discussion of the fallacy of assuming causality with pre-test post-test design see Donald T. Campbell & Julian C. Stanley, Experimental and Quasi-Experimental Designs for Research 7-12 (1966). Moreover, for purposes of the study the researchers must isolate attitude changes which are not only related to the plan but which are likely to be correlated with use of the plan.

[18] Since respondents were asked whether they were ever involved in various situations in which people frequently use lawyers, and if so whether they used a lawyer, it might have been possible to ascertain whether respondents during the plan year used lawyers in situations in which they had not used lawyers previously. However, the use of lawyers in situations where they had not previously been used would not necessarily be attributable to the plan. Only certain of the situations in which lawyers were used would turn out to be situations in which the same respondent had not used lawyers previously. Considerable time might have elapsed since the previous event. The respondent's financial situation might have changed in the interim. More significantly, he might have used a lawyer in the interim for something else. The data indicate that previous use of lawyers was the major predisposing variable to use of the plan; 78 per cent of those who used the plan had used lawyers previously. Hallauer at 228. It might have been that experience, rather than the plan, which accounted for the use of lawyers. The data suggest that only a fraction of the cases involved situations that the same respondent had faced recently yet in which had not used a lawyer. Even in those cases a special factor, such as the publicity given the plan, might have made the respondent temporarily more receptive to the idea of using a lawyer.

given time is about 600, there is considerable turnover. The original panel consisted of 502 out of the 583 eligible members of the local. "Because of member turnover and unavailability," the researchers were able "to interview with Schedule II only 301 members who had been interviewed with Schedule I." This meant that more than 200 first-wave interviews were wasted.[19] It also meant that the sample of those interviewed was weighted towards the more permanent members of the local. Because they were not available for the first wave of interviews the study does not include information about employees who came into the union after the first wave or who belatedly came under the plan or who left the union between the first and second waves. This turned out to be a particularly unfortunate omission because it meant that a sizable group of food handlers who later became eligible for the plan were not interviewed,

> This group . . . included a large number of white women and white men. . . . More important, . . . this group was involved in a lockout; and . . . events on the picket line . . . gave rise to multiple unemployment compensation claims . . . [and] multiple defendant battery actions both covered by the prepaid legal service plan.[20]

Had the researchers not chosen to utilize panel design they could have obtained information about the use of lawyers before and during the plan in a single post-plan interview. This would have increased the number of respondents about whom such information was available and thereby increased the ability of the researchers to make meaningful comparisons of pre- and post-plan use. Hallauer conceded that because of the small numbers involved, no comparative conclusions could be drawn about the nature of the problems for which clients saw lawyers prior to and under the plan. Had there been 600 rather than 300 respondents, some statistically significant changes might have been observed.[21]

Since the purpose of panel design is to permit the researchers to compare answers given prior to the experimental variable with those given afterwards, the post-experiment interview schedule is necessarily shaped by the questions asked beforehand. The addition of areas of inquiry afterwards is limited by the desire not to lengthen the interview schedule, which would mean greater interview and analysis costs and the possibility of respondent fatigue. Thus the research instruments are composed largely without benefit of experience

[19] Hallauer at 223, n.2; Marks, Hallauer & Clifton at 14.

[20] *Id.* at 16.

[21] In addition, panel design is expensive. Each interview involves separate costs for contacting the respondent, obtaining the interview, and recording, coding, transcribing, and producing the data received. This study involved interviews conducted over a period of a year and a half, which required the researchers to recruit and train two staffs of interviewers.

with the experimental variable. This problem can usually be minimized by running test studies in similar situations before designing the research instruments. However, the ability to conduct meaningful test studies prior to commencement of the Shreveport plan was limited because of the uniqueness of the experimental situation. To a considerable extent the research instruments were composed with little idea of which areas might produce useful results.[22] For example, it would have been interesting to probe the affirmative response given by 75 per cent of the respondents when they were asked whether they would join on their own, if necessary, a private plan involving the payment of similar fees.[23] One would also have liked to see further exploration of the response given by many respondents indicating that clients under the plan were treated better than they would otherwise have been. Were these responses related to pre- and post-plan experiences? If so, what were the experiences; if not, what were the responses based on?[24]

The Breadth of the Study

The researchers hoped that the plan would help provide answers to nine basic questions about the usefulness of pre-paid legal insurance. (1) How would the plan affect the use of legal services by its covered members? (2) Would the plan change the ways in which the clients selected lawyers? (3) Would clients have a free choice of lawyers in the open market? (4) Would the nature of the lawyer-client relationship be affected? (5) How would the lawyers be affected by the insurance plan? (6) What effect would the plan have on the attitudes of the covered members toward the legal profession? (7) Would the plan be economically viable at a reasonable premium? (8) What would be the attitudes of the groups concerned and of others toward the plan? (9) How well can the experience from this experiment be translated to similar programs elsewhere?[25]

The research instruments were shaped by the desire to obtain information about all of these questions. This limited the ability of the researchers to focus on any one aspect of the plan, something which would have been par-

[22] A large amount of space on the interview schedule was devoted to asking respondents what services lawyers performed for them before the plan and afterwards. These questions had little value. Respondents were not in a good position to answer them accurately, and the meaning of their answers was difficult to understand. Nor did the investigator really probe the nature of the respondents' experiences with lawyers.

[23] Hallauer at 241.

[24] 49 per cent so answered. *Id.* at 240. No correlation was found between an affirmative answer and use of the type of legal plan. Respondents were apparently asked in what ways they would be treated differently under the plan but there appears to have been little probing of the responses to this question and respondents were not asked to contrast pre- and post-plan treatment. See Marks, Hallauer & Clifton at 80-82.

[25] Marks, Hallauer & Clifton at 93.

ticularly desirable in light of the special nature of the situation. The less representative the sample, the more important it is to understand in detail the dynamics of the situation being studied. Such understanding would enable the researcher to predict in what other situations similar responses might be expected.

The questions posed were of such complexity that efforts to produce useful information about all of them in a single experiment were bound to fail and did. Thus the researchers conceded their inability to discover what the economic impact of such a plan would be.[26] This is an important and difficult question that would have justified a separate study. They also admitted, as mentioned above, that the special circumstances of the experiment "make it almost impossible for us to generalize from the data we were able to obtain."[27]

Even in those areas of the study where findings are reported they are generally skimpy and raise more questions than they answer, as the following examples should illustrate. In an effort to measure the impact of the experiment on the Shreveport bar, interviews were conducted with selected lawyers before the plan went into effect and with lawyers who had experience under the plan. While it is not clear what the researchers intended to study, it would have been interesting to learn how the plan affected the attitudes of lawyers toward handling cases for members of the local and the nature of the experience for them. The pre-plan interview with lawyers did explore to some extent their feelings about representing members of the local, but only a few questions were asked and they did not really go into detail about the issue. Lawyers were interviewed after the plan was in effect by an unstructured interview and it is possible that some information of this type was adduced but the reports thus far have been skimpy. The reaction of lawyers to the plan is described as ranging from "favorable to enthusiastic."[28] The major conclusion drawn is that the plan improved lawyer-client relations. The researchers and the members of the bar all concluded that the clients developed a "sense of entitlement"[29] because lawyers who had previously handled cases for poor black laborers, generally on a reduced fee schedule, now felt free to charge the union the normal amount for their services. Further exploration of this conclusion would have been desirable. The existing data are marred by being limited and impressionistic. Answers given by members of the bar

[26] Hallauer at 226-27.

[27] Marks, Hallauer & Clifton at 87.

[28] *Id.* at 88.

[29] During the pre-plan year 34 respondents saw a lawyer; 62 per cent said the major factor in their choice was advice of a friend or relative. During the plan year 59 people saw lawyers; 17 per cent reported it was mainly on the advice of a friend or relative. Community reputation went from three per cent to 12 per cent; advice of union officials went from zero to 22 per cent; and those who reported seeing a lawyer's name or office from zero to seven per cent. Hallauer at 232.

could be considered self-serving since the plan assures that the lawyers will be able to charge and receive their customary fee from poorer clients, and since the Shreveport bar endorsed the plan before it was put into effect.

Making Contact with a Lawyer

The study suggests that the method of making contact with lawyers changed. Many fewer people relied on friends and relatives to locate a lawyer. Instead a considerable proportion contacted lawyers through union officials. The percentage reported as relying on community reputation increased, as did the percentage that contacted lawyers randomly.[30]

Hallauer concludes that a new system of referral was at work. "The informal contact system did not break down altogether, rather it was replaced by a somewhat more formal one," which, he concludes, "is probably better than having to rely on the random experiences of a friend or relative . . . but it is not yet free choice of one's lawyer if that phrase means a careful and informed weighing of alternative legal resources."[31] Neither the optimism nor the pessimism are justified on the basis of the data presented. There is no reason to believe that union officials in this situation are better able to select adequate lawyers than are friends. Community reputation, which is not defined, would seem to be an overlapping category with referral by friends; further probing would be necessary to determine the ways in which they differed. Random selection, which increased during the plan year, would seem to be less desirable than previous methods of selection. The conclusion that the plan led to a more careful system of selection is not supported by the data, while the goal of informed choice, as that concept is defined in the quoted language, seems remote and unlikely to be achieved by any delivery plan. As Hallauer recognizes, few people, regardless of class, select in this manner. It is accordingly difficult to understand the importance given this shift by the investigators.

Attitudes Toward Costs

Before the plan was in force employees were asked whether they agreed with the statement that most of the lawyers charge too much. The vast majority—87 per cent—agreed. After one year of the plan 85 per cent agreed.[32] The results are not surprising. As Hallauer suggests, they are irrelevant to the success of the plan. He states that a well designed plan should result in "measurable positive attitudes about the plan and the delivery sys-

[30] *Id.* at 231.
[31] *Ibid.*
[32] *Id.* at 236.

tem as distinct from the lawyer."[33] A good point, but then why ask the question, particularly since it seems possible that almost as many people in any walk of life, including those who use lawyers regularly, would agree?

Before the plan was in effect respondents who had been charged legal fees were asked what the legal fees were and whether they could afford them. A substantial proportion said no, they could not; as the fee rose, more said they could not. Where the fees were between $11 and $25, 12 per cent said they could not afford them, but for fees greater than $300, 58 per cent answered no.[34] It is hardly surprising that a substantial number of poor people indicated that they could not afford a fee of more than $300, but the significance of the answer is not clear since all of these people had gone to lawyers and more than 80 per cent indicated that they would use the same lawyer again. The problem is that the question was an ambiguous one. It is not clear what the answer that one could not "afford" a particular fee meant. Perhaps it meant that the respondent thought the fee was exorbitant, perhaps that it imposed a financial burden. Respondents were not asked their views about the adequacy of the services they received nor whether they thought the fee justified or earned. Hallauer obviously assumed that a negative answer was equivalent to a statement that the lawyer charged too much and he concludes that the 80 per cent who answered that they would use the same lawyer again were "locked into an ongoing relationship with the same lawyer."[35] It might equally well indicate general satisfaction with the work done and a feeling that the fee, although difficult to pay, was justified. Since the best predictor of use of the plan was having gone to a lawyer previously, it would seem more likely that respondents were generally satisfied with the help they received before the plan.[36] It should be noted that respondents who had no previous experience with lawyers were more likely than those who did to answer that lawyers' fees are too high and that this answer was not affected by the size of the previous fees.[37]

CONCLUSION

Although the unique features of the Shreveport plan made it inevitable that the results would not provide a basis for generalization, the plan afforded a

[33] *Id.* at 234.

[34] *Ibid.*

[35] *Id.* at 235.

[36] *Id.* at 227-28.

[37] *Id.* at 236-37. One area that was left unexplored was the impact of the institution of the plan on attitudes towards the union. It would be useful for the union to know whether employees considered legal insurance a desirable and important union goal to strive for in collective bargaining with employers and whether workers' attitudes toward the union changed.

useful opportunity for a particular experiment. The opportunity could have been better utilized if the researchers had used a control group and if they had considered the advantages and disadvantages of panel design more carefully. It would also have been desirable to have the respondents focus more intensely on fewer areas of inquiry. For research conducted after the plan was in operation, test studies would have been most useful. The tentative conclusions favorable to the plan suggested by Hallauer are not supported by the results reported.